tion, its maximum occurring in June and July, and its minimum in December and January.

But that such a result might not rest merely on the observations with one instrument, the author has collected observations made on corresponding days at the observatories of Greenwich, Dublin, and Paris, all which, without exception, give results very nearly agreeing with those of his own observations; and he finally concludes, that as they cannot possibly arise from instrumental error, or error of observation, he hopes to be able ere long to prove, to the satisfaction of the Society, that their cause is imperfection in the solar tables.

On the Existence of a Limit to Vaporization. By M. Faraday, *F.R.S. Corresponding Member of the Royal Academy of Sciences at Paris, &c. &c. Communicated May* 26, 1826. Read June 15, 1826. [*Phil. Trans.* 1826, *p.* 484.]

The object of this paper is stated by its author to be, to show that a limit exists to the production of vapour of any tension by bodies placed in vacuo, or in elastic media, beneath which limit they are perfectly fixed. The train of argument by which this is attempted to be demonstrated may be summarily stated as follows.

Assuming it as proved by Dr. Wollaston, that a limit exists to the earth's atmosphere, where in consequence of its rarity its molecules are so distant from each other, that the repulsion of any one on the molecule below it is just equal to its gravity, it is clear that in this case the force of gravity may be regarded as setting a limit to further expansion; and if we could exhaust a receiver to the degree of tension obtaining on the surface of the atmosphere, any further subtraction of air would produce no further diminution of density, but would merely produce a vacuum in the upper part of the receiver.

But cold, as well as rarefaction, diminishes the elasticity of vapours or gases, and therefore if the temperature be greatly diminished, the limit above alluded to, where gravity counteracts the elastic force, will be attained with a less degree of rarefaction; and if the temperature were sufficiently low, it is evident that air of any given degree of density would lose its disposition to expand in a direction contrary to gravity.

In the case of air, however, the cold required to render it inelastic at any sensible density would of course be excessive. But if we consider the vapours of very fixed bodies (as silver for instance), whose tension even at a white heat is insensible, it is almost certain that the ordinary temperature of the atmosphere is, with respect to that capable of maintaining it at a sensible tension, such a degree of cold as would effectually bring it under the command of gravity. Supposing then silver to cool from fusion, that moment when these forces became equal, would be the one in which vapour could exist *above* the silver; and at every lower temperature the metal would be perfectly fixed. But the author regards it as probable that this equilibrium at ordinary temperatures may take place with bodies

much more volatile than silver, and states an experiment made with mercury in the winter of 1824-25, where no action on gold-leaf, suspended over it, however near, took place, from which he concludes the mercury then to have been perfectly fixed; and other experiments on mercury and on sulphuric acid by Sir H. Davy and Signor Bellani are adduced in support of the same view.

But there is another force, that of homogeneous attraction, which the author regards as sufficient to overcome a certain degree of vaporous elasticity; and he illustrates the mode of action of this force by an experiment on the slow crystallization of camphor, and by that of other substances from vapour in the process of sublimation; and by analogous phenomena in the crystallization of salts from aqueous solutions.

On Electrical and Magnetic Rotations. *By* Charles Babbage, *Esq. F.R.S. &c. &c. Communicated May* 29, 1826. Read June 15, 1826. [*Phil. Trans.* 1826, *p.* 494.]

The author first recapitulates the manner in which he conceives time to influence the results of the magnetic phenomena observed by M. Arago, and which need not here be repeated, being in substance that given in a paper on the subject, in the Transactions of last year, stated in a more geometrical form. As the reasoning in this argument requires only that an attractive or repulsive force should be communicated from one body to another in a finite time, it occurred to him that electricity might be substituted for magnetism, and that rotations analogous to those observed by M. Arago might be produced by the use of electrified instead of magnetic bodies. He accordingly suspended by a fine silk thread a thin brass bar with circular ends over a disc of glass; and the bar being electrified by contact with excited sealing-wax, the glass was made to revolve slowly, when the bar was observed to be dragged round in the same direction. The effect was decided; and all proper precautions were taken to avoid disturbing causes, such as currents of air, twist of the silk, &c. The effect was greatest with a slow velocity of rotation, about five turns in a minute. On substituting a stick of excited sealing-wax for the brass bar, the same effect was produced; but when the rotation of the plate was rapid, the stick remained nearly immoveable. The same effect was produced when the glass plate was covered with plates of copper, lead, or other metals cemented to it.

A proper apparatus being constructed for the purpose of further experiments, an excited electrophorus was made to revolve under a flat needle of thin brass with circular ends, with various degrees of rapidity. The motions of the needles were irregular and complicated, but appear to the author capable of explanation, as well as the others on the same principle,—that electricity excited by induction is not instantly destroyed by removing the inducing body.